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ABSTRACT

Research strategies used to study human aggression include laboratory study, experimental simulation, field experiment, field study, judgment task, sample survey, and less empirical strategies such as computer simulations and formal theory. The context of these strategies can be classified as either contrived, natural, or irrelevant. Major issues revolving around the context deal with the generality of findings and control over extraneous factors. Precision of measurement reflects the clarity with which a dependent variable can be operationalized and measured. Although the laboratory study gives maximum control over the measurement of behavior, the issue focuses on the abstractness of the operational measure and its validity. Closely related to the issue of precision of measurement are the areas of research designs and statistical analysis. Analysis of variance is the method most widely used: other designs include nonorthogonal, experimental, and designs that contrast a within subject to a between subject version of a variable. The issue of the theory/application balance, termed ecological validity, relates to the generality of conclusions across persons, settings or specific behaviors. The three major goals of research (precise measurement, realism and generality) cannot be achieved in studies using only one paradigm. (NRB)

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NATURALISTIC VERSUS EXPERIMENTAL APPROACHES TO THE STUDY OF HUMAN AGGRESSION: THEORETICAL AND METHODOLOGICAL ISSUES

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Naturalistic versus Experimental Approaches to the Study of Human Aggression: Theoretical and Methodological Issues

The purpose of the present paper is to examine the state of the current methodological approaches to the study of human aggression. It will be argued that the various approaches are asking different questions, and that the strengths of one approach are the weaknesses of the other. Cronbach, (1957, 1975) writing on the two disciplines of psychology, raised issues concerning the conflicts between the experimentalists and correlationists. These issues seem as alive and relevant today as they were 25 years ago, and are quite pertinent to the aggression researcher and include the control of behavior under specified conditions versus the predictability of behavior across time, along with the search for general laws versus the interest in interactions.

When Kenneth Bowers (1973) said, "Thus doth method move our minds to differing perceptions of reality," he was characterizing the way in which experimental approaches to studying behavior lead us to conclude that behavior is predominantly under environmental control, and that to change behavior we need only change the situation. On the other hand, more correlational methodologies would lead us to believe in the enduring, stable characteristics that reside within individuals and persist across situations. What is clear here is that two different questions are being addressed, and it is unlikely that either method can adequately answer the questions being raised by the other.

Runkel and McGrath (1972) presented a model for classifying various research strategies one can use. This model illustrated the main distinguishing features of each strategy, along with their strengths. The main facets were

the setting (contrived, natural or irrelevant), the obtrusiveness of the measurement, and whether one was examining concrete real life behaviors or analogs of real world behaviors. Weaknesses of any one strategy were represented as the strengths of another strategy. The present discussion deals with four major points regarding the various strategies: the research context; the precision of measurement; data analysis; and the theory-application balance as it relates to the generality of findings. A brief description of each strategy will be followed by a discussion of each of these points.

Research Strategies

Laboratory Study. Most representative of the laboratory approach to aggression are those paradigms that involve a subject's delivering some form of noxious stimulation to a target/victim, following exposure to some experimental manipulation, or set of manipulations, such as being verbally insulted, physically attached, frustrated, being exposed to aggressive or erotic stimulus materials, or being administered some quantity of alcohol. Examples include variations of Buss's (1961) effects-of-punishment-on-learning context; Berkowitz' (1962) evaluation-of-performance format; Taylor's (1967) competitive reaction time task; and Gaebelein's (1973) third party instigation procedure. In these types of studies there is experimenter control over the assignment of subjects to conditions, precise definition of independent variables and careful measurement of dependent variables. Data analysis is typically the analysis of variance.

Experimental Simulation. An experimental simulation is illustrated in Bryant and Zillmann's (1979) study in which students in a classroom ostensibly were asked to make suggestions to the instructor concerning who should receive a guest lecturer appointment, following a number of manipulations designed to

arouse various levels of hostility. While this strategy closely resembles the laboratory experiment, it is distinguished by its emphasis on a concrete behavior in a more closely approximated naturalistic setting. This setting affords more vigorous control than the field experiment.

Field Experiment. Field experiments, which resemble laboratory experiments except they take place in natural settings, include procedures such as those used by Mary Harris: people bumping into others, cutting in line, and blocking intersections (Harris, 1973) along with making wrong number phone calls (Harris, 1974). These types of studies permit a random assignment of subjects to conditions, though control over the independent and dependent variables is not great; frequently the analysis of data is less vigorous. Chi square to analyze frequencies is typical.

Field Study. Many of the studies that look at the effects of media violence on behavior fall in the field study camp, which involves systematic observation within an intact behavioral and social system, being as unobtrusive as possible. Eron's work (Note 1) is representative. Observational studies of aggressive behavior of children also fit into this category. Studies conducted by both Barrett (1978) and Jacklin and Maccoby (1978) are excellent examples. Jacklin and Maccoby observed the total social interaction pattern of 33-month old children in same and mixed sex dyads. Their study was controlled to the extent that they created the dyads and had given parents instructions in how to dress their children, i.e., comfortably, no dresses for girls. Barrett's study, on the other hand, was totally observational, in that he had observers record samples of 5-8 year-old boys and girls at a summer day camp over a period of six weeks. The rigor in a study such as this comes at the level of the observational protocol used and the sophistication of the data

analysis. Independent variables, though not manipulated, were identified and included settings in which the child played, sex of the initiator of a social interaction, and sex of the recipient. Most investigations of family violence are also naturalistic in approach, relying on observation and/or questionnaires and interviews (Straus, Note 2).

Judgment Task. A number of studies that look at the perceptions of, and labeling of, aggression can be classed as judgment tasks. In this strategy the setting is usually irrelevant. Subjects are asked to make judgments about stimuli presented to them. The behavior of interest is neither intrinsic to the setting nor is the researcher interested in judgment behavior per se. Rather, the interest is in that which is being judged. For example, Frodi (1977), using open-ended questions, found that a given provocation was perceived differently by males and females. In a similar vein, Tedeschi, Smith, and Brown (1974) reported data that indicated that whether or not an act was judged "aggressive" depended upon the context, and preceding events.

Sample Survey. A sample survey approach, which is also not setting-dependent, is illustrated in a study by Duncan and Hobson (1977). In their survey they polled 933 males and females from a variety of backgrounds, students, teachers, nurses, social workers, realtors, housewives, and armed forces personnel, concerning their views on the behavioral characteristics of aggressive and nonaggressive acts among males and females.

Other Strategies. Runkel and McGrath (1972) include computer simulations and formal theory as research strategies, although these approaches are not empirical. They are, however, included since they are means to systematically processing and deducing or inducing information. An important point here is that these methods would have little use without the data obtained via the

others; and vice versa. Empirical data collected without some guiding theory would be trivia (Bavales, 1978). Other research strategies available derive from a sociological perspective and represent amalgamations and/or extensions of the strategies discussed above. For example, structured observations of behavior may be made in the laboratory, in the absence of any experimental manipulations. Contextual analyses use states or countries as the unit of study. An additional approach can examine the phenomenology of aggression by examining the meaning of aggressive acts.

Researchers using each of these research strategies were asking questions more appropriately approached by one method than other. To use a distinction made by Petrinovich (1979) some strategies tell use about the possibility of a relationship and some tell us about the probability. The issue should not be, is the laboratory or the field a better place to conduct research. Rather a researcher should ask, which is the best strategy for testing a given hypothesis or hypotheses. Even better, one should ask how can I approach this problem using several different research strategies to lend support to my conclusions.

Context Considerations

The context defines the physical environment within which data are collected. Settings may be contrived, as in a laboratory study, or natural as in experimental simulations, field experiments and field studies. Some research methods would consider the context irrelevant, such as in sample surveys or judgment tasks. The major issues revolving about the setting deal with the generality of findings and control over extraneous factors. The researcher may be concerned with whether or not the situation is sufficiently similar to real life that the results can be applied; in other words, do the

data have ecological validity? Obviously, the contrived and arbitrary nature of many laboratory set ups may interfere with generality to the real world. However, in demonstrating the functional relationship between variables this may not be a major concern. That is, in demonstrating the causal relationship between a variable and a behavior/attitude/emotion, the researcher is typically more concerned about eliminating any rival hypotheses that may explain the relationship than to have a totally realistic situation. Hence, it becomes critical to have a high degree of control over as many details of the setting as possible. Here the contrived nature of the laboratory is to the researcher's advantage.

Clearly the laboratory setting provides the researchers with the most precise control of variables. This opens the experimentalist to the major criticism of this approach: the contrived, artificial nature of the situation and accusations of being unethical if one is devious (or clever) enough to come up with a believable situation within the experimental context. The argument is that the laboratory setting is so far removed from the subject's everyday experience that behavior is such a context can tell us nothing about the real world. For example, Berkowitz and LePage's (1976) study of the weapons effect came under severe criticism as being merely an experimental artifact. This controversy was somewhat abated when Turner, Layton, and Simons (1975) were able to replicate the effect in a field setting. Rarely do any of us go around delivering electric shocks or bursts of aversive noise to randomly selected strangers that we will never see, or at least never see again. On the other hand, it has been argued that although the field may provide us with a more realistic setting to examine the things people <u>really</u> do to each other, or try to do, there is little control for the extraneous

variables that may account for one's actions. Perhaps this is why a wider range of behaviors are studied in naturalistic contexts and labeled "aggression." For example, we see studies of verbal insults, performance evaluations, hostile glares, passive refusals to help, horn-honking, and gossip, all of which are called aggression. Here too one finds various criminal acts, including child and spouse abuse, rape, assault, and murder, being investigated.

The setting within which we conduct our investigations can affect substantially how general our conclusions can be. When certain methodological changes in an experimental procedure change behavior, we need to question the power of such a variable to account for much of the variance in that behavior in the real world. The concept of "inexact replication" is applicable here. Precisely for the reason that we cannot hold conditions constant from one replication of the study to the next in the field setting we have stronger evidence for the power of the variable under consideration; a variable needs to be relatively strong to be significant even when a number of extraneous factors are operating (Kerlinger, 1973). However, we cannot sacrifice internal validity for the sake of realism. On the other hand, in the laboratory even seemingly minor changes in the procedure can produce significant effects. For example, in a study I conducted (Gaebelein, 1978), using the third party instigation paradigm, half the subjects were given a shock button labeled "zero," meaning no shock, in addition to five more buttons representing varying intensities of shock. The remaining subjects did not have this "no shock" option. This procedural change altered considerably subjects' perception of the task, and what had previously been considered nonprovocative behavior from the target became provocative. Similarly, in a series of studies on modes of social influence (Burton, Note 3; Gaebelein, Note 4), I have found

that when subjects are given a variety of response options, the aversive modes of influence (i.e., electric shock, loud noise) are not the most preferred; furthermore, males and females show remarkably similar overall patterns in responding. However, when we focus in on the sex differences in just those modes traditionally considered "aggressive," males appear more aggressive than females. It was clear from these studies that the context within which we view a behavior influences our interpretation. If we look at just aggressive behaviors we see sex differences, but if we look at the entire range of behavior males and females are far more alike than they are different.

In all our laboratory studies of aggression we need to ask ourselves, "In generalizing my results what contextual factors must I account for, such as the other behavioral options an individual could have (i.e., leave the situation), which I did not allow for in my study?" Conversely, the field researcher needs to ask, "What uncontrolled variables in this natural setting could provide rival hypotheses to the factors I have been investigating?" We cannot pay the price of losing internal validity in order to obtain realism.

Definition/Measurement

Precision of measurement reflects the clarity with which a dependent variable can be operationalized and measured. For example, intensity of electric shock chosen for a target victim can be measured more accurately and precisely than severity of child abuse.

While all research strategies must pay attention to the definition of aggression, the laboratory study gives the researcher maximum control over the measurement of the behavior of interest. The issue becomes one of how abstract, or how far removed from the behavioral concept of interest, can the operational measure be and still be valid. To what extent can we generalize from a precise

laboratory-assessed behavior to the real world of harm-doing?

In examining the nature of the aggressive acts studied under the various research approaches outlined above, we are faced with the fundamental question of what is aggression; what constitutes an aggressive act? Most laboratory researchers seem to be in general agreement that aggression is the delivery (or attempted delivery) of a noxious stimulus to another. Kaufman's (1973) definition of aggression as "behaviors which: (a) are transitive, that is, are directed against some object; (b) have a subjective probability > 0 - on the part of the attacker - of reaching that object, and either removing it from the attacker's goal path, or imparting a noxious stimulus to it, or both" (p. 353) seems to be operative, at least implicitly, for many current researchers. However, the operationalization of this definition varies somewhat from laboratory to laboratory. Most of these operations involve delivery of electric shock, aversive noise, or negative verbal statements. Ideally, these operational definitions are analogs, or abstractions, of what is called aggression. If this is a valid assumption then it does not matter that people really do not deliver electric shock or bursts of aversive noise to each other in the real world. We can interpolate from these behaviors in laboratory to the real world.

On the other hand, when we are interested in a concrete, specific form of aggression, then our measure must be of precisely that behavior. Perhaps this is one reason why there is a wide range of behaviors studied in the field and called aggression. Also, there does not seem to be as great a consensus on definition or measurement of aggression among field researchers. While the field researchers may be precise in the observational codes they use, the underlying conceptualization of aggression is not always as specific. We can

ask legitimately does the rough and tumble play of children really reflect a desire to harm another; is hitting a Bobo doll really aggression (what else does one do to a Bobo doll?); is harm-doing actually involved in horn-honking or giving someone a dirty look? What are the aversive consequences for the target of such acts, and was intention to harm involved? Frequently in field studies, judgment tasks, and sample surveys, hostile attitudes and fantasies are measured. Occasionally researchers treat these as aggression; others treat them as only correlates. The relationship between attitudes and fantasies and aggressive behaviors needs to be directly addressed by those who use such measures.

Clearly, those who do observational studies have the advantage of not being harnessed by ethical considerations in defining aggression to the extent the laboratory researcher is. Obviously, abusive, mutilating, life-endangering acts cannot be studied in the laboratory. There are ethical restraints on the range of behaviors we can study in the laboratory. At issue is whether or not our operational definitions of aggression, such as "the delivery of unpleasant though not harmful electric shock," are adequate analogs of real world aggression.

A major step in the direction of measuring aggression more precisely would be to recognize explicitly that aggression is not a unitary concept. What is needed is an agreed-upon typology of aggressive acts. A scheme that would allow us to identify the types, purposes and topography of various aggressive behaviors would be a great aid to both the laboratory and field researcher. Attempts to do this in the past have been made, though no one system seems to have caught on. Berkowitz (1962) made the "instrumental-expressive" distinction; Buss (1961) delineated aggression in terms of direct-indirect, active-passive,

physical-verbal; and Tedeschi, Smith and Brown (1974) have asked us to reconceptualize aggression as the exercise of coercive power. Most recently, Rosenzweig (1977) has suggested a model for classifying situations that trigger aggression. Until there is more consensus about what it is we are studying, we will continue to have that sense that "the work one researcher is doing within one paradigm has little to do with what I am doing with my paradigm." And the "outsiders" will continue to wonder what we psychologists have to say about those social issues that concern them the most: child and spouse abuse, rape, murder and war.

Data Analysis

Closely related to the issue of precision of measurement is the discussion of research designs and statistical analysis. As we are all probably aware, the analysis of variance has become the primary form of analysis for data in factorial designs (Rucci & Tweney, 1980), so much so that we tend to teach our graduate students that the best way to answer a question is to design an experiment that will fit the ANOVA model. This kind of thinking has been questioned recently. Platt (1964) wonders if some scientists have become more method-oriented than problem-oriented. He talks about the "one man-one method" pattern. This seems almost rampant in the area of aggression research. Many researchers are most highly associated with a single paradigm rather than a problem area. Fortunately, this is beginning to change. Petrinovich (1979) questions whether or not the additive, linear model we assume is operative in the ANOVA is really applicable to complex real world behavior. For example, it would be ludicrous to try to take even a partial list of those variables Eron and his associates (Eron, Note 1) have identified as predictive of aggression and put them into a factorial design. Imagine, for example, trying

to interpret an exposure to TV violence X parental interpretation of TV content X maternal warmth X paternal punitiveness X masculine-feminine interests X sex of the person. Obviously, these are not true independent variables in the sense of being manipulable, with experimenter control over random assignment of subjects to conditions. But that's just the point. Some of the relationships we consider important in our study of aggression do not lend themselves to the ANOVA. Regression analyses and various other multivariate approaches are more appropriate for testing such hypotheses. Many of us probably need to leave the familiarity of the ANOVA model (with equal sample sizes in all cells) and become students again long enough to learn the uses of the correlational and multivariate techniques that computers now give us easy access to. Though the laboratory experiments give us the best chance to collect data that conform to the various mathematical assumptions that underlie parametric statistical tests, we need to recognize that there are ways to control statistically for variables we cannot experimentally control, i.e., analysis of covariance. Various forms of regression analysis, path analysis and cross-lag correlations are other examples.

Because of space limitations, we can't go into details about all the design options available to us, so I just want to mention a few designs that might be worth considering: nonorthogonal designs (Herr & Gaebelein, 1978) with sample sizes chosen to reflect true population proportions; quasi-experimental designs (time series, multiple baseline, etc.) (Campbell & Stanley, 1963); designs that contrast a within subject to a between subject version of a variable (Erlebacher, 1977). Regarding this last design strategy for example, we know that high attack elicits aggression but whether a high level of provocation precedes or follows a low level of attack alters subject's aggressive

behavior. This has been shown in laboratory studies by O'Leary and Dengerink (1973) and Gaebelein (1978). The point is whether or not a variable is a between subject factor or a within subject variable changes a subject's response. This must be considered in data interpretation. Finally, designs that permit the analysis of several dependent variables simultaneously may help us to get closer to identifying a typology of aggressive behaviors, i.e., what specific behaviors covary under what conditions? For example, a number of researchers are beginning to include several measures of aggression in their study. In a study by Gaebelein and Hay (1974) we found that subjects' verbal aggression was affected by the independent variables differently than was physical aggression. Similarly, researchers have found that the intensity of shock and the duration of the shock do not necessarily covary.

Maximizing generality: The theory/application balance

The final point to be discussed relates to the generality of our conclusions across persons, settings and specific behavior. This issue has been called external or ecological validity.

It seems that most research on aggression in the laboratory camp can be distinguished from that in the naturalistic camp on a theory-application dimension, although I want to make it clear that this distinction is far from a pure one. As the experimental approach to aggression has evolved since Buss's (1961) and Berkowitz' (1962) first books on aggression, each of the major theoretical positions has become pretty well associated with a name, a paradigm and specific hypotheses associated with a specific method. For the most part all of this work has used the laboratory experiment format. Buss has shown us the instrumental nature of aggression; Bandura has illuminated the social learning principles of modelling and imitation; Berkowitz has explored the role

of arousal and aggressive cues; and Zillmann has a model for the transfer of residual excitation. Researchers continue to examine the limits of the frustration-aggression hypothesis (Rule & Nesdale, 1974) and the concept of catharsis (Konecni, 1975), as well as the basis for sex differences in aggression (Frodi, et al., 1977; Richardson, Note 6; Gaebelein, Note 4). The examination of these phenomena in the laboratory typically using college students makes our critics wonder what we have to say about the real world.

On the other side of the coin, researchers interested in social issues have traditionally done more naturalistic studies on topics such as family violence (Gelles, 1974; Steinmetz, 1977; Straus, 1977). TV violence research (Eron & Huesmann, Note 1; Singer, 1971) has a tradition of being more naturalistic, though certainly both Bandura's (1973) and Berkowitz' laboratory work has contributed greatly to this area. The critics of the field approach may say "Fine, you have described some relationships, but we want to know what causes what? What underlying principles of behavior have you revealed?"

To answer our critics who claim our laboratory studies are so artificial as to negate any ecological validity, we need to be cognizant of the ways in which laboratory data and observational data fit or contradict one another. On the other hand, proponents of the field approaches need to pay more attention to the various extraneous factors that may hold a causal link in an observed correlational relationship between aggressive behavior and some independent variable. For example, crime statistics show a strong relationship between alcohol and aggression, and Taylor's experimental studies confirm this relationship under well-controlled conditions. However, while his drunk subjects in the laboratory are fairly uniformly aggressive when provoked, nightly on the streets there are drunks who are not aggressive. What are the

important individual and contextual variables operating in the real world that may attenuate the alcohol-aggression relationship for at least some of us? To give another example, we see crime statistics that demonstrate how violent some women can be, yet the typical Buss-type laboratory study seems to find that males are more aggressive than females. Only quite recently are researchers attempting to identify factors that minimize and maximize differences in the aggression of men and women.

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Footnote

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